groups of chemicals have been discovered which are efficient in the treatment of trypanosome infections. They are: (*a*) benzidin dyes, (*b*) basic triphenyl-methane dyes and (*c*) arsenical compounds. In experimental animals complete cure has apparently been effected by maximum doses of these compounds. With lesser doses and prolonged treatment the parasites may disappear from the blood for a time, but later on make their appearance again. Those which recur have undergone a pronounced change in their biological characters and constitute a strain resistant to the therapeutic agent employed. Such a strain manifests chemoresistance of a specific character towards the particular substance used to develop it and an increased resistance towards other compounds of the same group. On the other hand, the development of resistance towards one group causes no increase whatever in the resistance towards other groups. By continued experiments, however, a strain has been produced manifesting a triple resistance, specific towards each of substances employed.

Chemo-resistance, once acquired, persists unchanged while the resistant trypanosomes are passed through normal animals even for one hundred and forty transfers extending over fourteen months. This has been cited as strong evidence of the transmission of acquired characters. The specificity of the resistance is very striking. After an experimental animal has been inoculated with a mixture of two resistant strains and is then treated with a substance towards which one of the elements is resistant, the other element will disappear from the blood, but the resistant strain will remain and develop unchecked. Indeed, the two strains remain separate and capable of isolation after repeated passages through infected animals. Or, in other words, a strain with double resistance or with modified resistance does not arise as the result of infection with a mixture of two resistant strains.

HENRY B. WARD.

ANIMAL BEHAVIOR

Recent Work on the Behavior of Higher Animals.—There exist to-day two main centers for the strictly scientific and experimental study of the behavior of the higher animals. One is at Harvard, led by Yerkes, the other at Chicago, under Watson. Excellent work appears at times from other quarters, but it can usually be traced to the influence of one of the two men named. There is a third independent center for such work at Clark

University, but lacking the single-minded leadership of the other two, the attack on the problems has there been less unified and effective. Thorndike, whose work some years ago gave such impetus to the whole subject, has unfortunately been drawn into other work, or we should doubtless have another most effective center for such investigations. Outside of the United States the scientific study of the behavior of the higher animals is a negligible quantity, compared with what comes from the centers named. The active French movement in comparative psychology, under the influence of Bohn, Piéron and others, has been thus far limited mainly to the invertebrates.

At the laboratories we have named the work on animals is carried on by the aid of such accurate appliances and methods as have long been developed for the investigation of the physiological psychology of man, with ingenious modifications and additions as required by the peculiarities of the subject. This gives the work an almost uniquely precise and scientific character, as compared with most other studies in animal behavior. Most other workers have been compelled to content themselves with apparatus, observations and experiments of a more "homemade" character. The two leaders, one by training a zoologist and psychologist, the other a physiologist and psychologist, have been devoting themselves largely to rats and mice of late, while followers have made side excursions into the territory of cats, dogs and raccoons. It is necessary to concentrate the attack somewhere, and for the present the rats and mice are bearing the brunt. We shall pass in review the recent contributions from the centers named, limiting ourselves at present to work influenced from Harvard.

From Harvard we have first the elaborate study of the dancing mouse, by Dr. Yerkes.[1] Perhaps the most striking feature of this work lies in the elegant and fertile methods devised by the author, and of course in the results attained by these methods. The main method consists in a sort of "Lady or the Tiger" alternative presented to the unsuspecting mouse. He is invited to enter one of two doors; one leads to an electric shock, the other to freedom and food. The fateful portals are marked with signs of various sorts,—cards of different shapes, markings, color, brightness, odor, etc. The "right" and "wrong" doors can be alternated at the will of the experimenter, as can

[1] Yerkes, R. M. The Dancing Mouse, a Study in Animal Behavior. The Animal Behavior Series, Vol. I, 290 pages. The Macmillan Co., 1907.

the signs. The mouse in repeated experiments tries at first the simple plan of returning to the right or left door according as he has found that to be correct. When he finds that the correct portal is being alternated, he quickly learns to alternate in his choices. But when he finds that there is no regularity in the alternations, he begins to pay careful attention to the signs posted about the portal; "to run from one to the other, poking its head into each and peering about cautiously, touching the cardboards at the entrances, apparently smelling of them, and in every way attempting to determine which box could be entered safely." Often the mouse runs from one portal to the other twenty times or more, before deciding which to enter. Now, it is in this state of uncertainty and concern that the mouse is ready to give interesting results in animal education and in sense physiology. He uses all his senses to the best of his ability in determining which is the "right" door to enter, so there is opportunity, which Dr. Yerkes has skilfully used, to test his senses, and at the same time to study his ability to learn. When the two portals are indicated, the "right" one by a light card, the "wrong" one by a dark card, the mouse learns to choose the correct card. If for both cards are substituted others that are of deeper shade, but have a similar relative brightness, the mouse continues to choose the one of lighter shade. He has learned, not that a particular card, or a particular shade, is the right one, but that the *lighter of the two* is the one to choose; he often runs back and forth many times, seeming to compare them carefully. By accurately grading the difference in brightness between the two portals, it was possible to determine just what differences the mice could discriminate, giving an opportunity for work on Weber's law. *The mice rapidly learned to discriminate finer and finer shades of difference.* A certain mouse, in a first series of experiments, could discriminate only when the difference in brightness was practically half the greater brightness. In a later series he could discriminate when the difference was but one fifth and, after much more practise, when the difference was only one tenth. Such educability at first carried confusion into the data designed to test Weber's law. When it was finally taken into account, the law was found to hold.

In a similar way Yerkes made extensive studies of color vision in the mouse. He found that apparently they do not see colors,

at least not as we do; that most of their apparent discrimination of color is due to differences in brightness; and that the brightness of different colors is not the same for them as for ourselves.

It is extraordinary that the mice were unable to discriminate the portals by different shapes of cards or of lights. They showed no power of distinguishing forms.

We have given some samples of Yerkes' methods and results; many other matters of equal or greater interest were studied, by varied methods, including the classic one of using labyrinths. The author's experiences are set forth in interesting chapters on educability, methods of learning, the efficiency of different methods of training, the duration of habits, the revival of lost habits, individual differences in behavior, and the like. When it comes to responding to experimentation, the dancing mouse is, as its name indicates, rather an artistic than a strictly utilitarian animal, giving a delightful variation from those orthodox creatures whose main desire is to "get there," so that results are not readily expressed correctly in terms of minutes required and space passed over. "Most mammals which have been experimentally studied have proved their eagerness and ability to learn the shortest, quickest, and simplest route to food without the additional spur of punishment for wandering. With the dancer it is different. It is content to be moving; whether the movement carries it directly to the food-box is of secondary importance. On its way to the food-box, no matter whether the box be slightly or strikingly different from its companion box, the dancer may go by way of the wrong box, may take a few turns, cut some figure-eights, or even spin like a top for a few seconds almost within vibrissa-reach of the food-box, and all this even though it be very hungry."

In addition to the strictly experimental work, Yerkes gives a full account of the peculiar "dancing" movements that have given the animal its name; a sketch of what we know of its history, and an extensive discussion of the disputed question as to whether its ears are defective and whether it is deaf. Yerkes concludes that it can hear only for a few days, when about two weeks old.

Altogether, Dr. Yerkes' book is one of the most attractive as well as one of the most valuable of the strictly scientific studies of animal behavior. It would be venturing out of the

"strictly scientific," but one wishes that the author might give us an imaginative picture of what life and the universe may be in the consciousness of this little creature, that does not hear, sees little or nothing of colors, can't distinguish a square box from a round one nor a circular card from a triangular one, feels impelled to "cut figure eights and spin like a top" on its way to a dish of food, and learns many things rapidly and well. Possibly such an unscientific picture could be appended to the really scientific account without injury to the latter! Dr. Yerkes is still studying the dancing mouse, and may some time feel prepared to give us such a picture.

Or perhaps we must look for such pictures to the second volume of the Animal Behavior Series, of which this is the first! The second one, just announced, is a volume on "The Animal Mind," by Margaret Washburn. The Series, edited by Dr. Yerkes, promises to be of the greatest value.

A matter that has been most in need of study is the part played by imitation in the behavior of higher animals. Years ago imitation was the favorite refuge of those who wished to explain the remarkable actions of animals without attributing to them higher intellectual powers. When Tabby pressed the latch and walked out the door, that was because she had seen some one do it. Then came Thorndike, and changed all that. To give imitation in place of reason as an explanation, says Thorndike, is to substitute one false explanation for another. In studying cats and monkeys, Thorndike saw no signs of imitation either of one another or of man. And most later investigators have agreed that imitation plays little part in the behavior of animals, at least in comparison with what had been supposed. Even the monkey, we are told, rarely imitates man or other monkeys. Direct, unreflective imitation of simple sounds or movements—the performance of an act merely because a companion has performed it, without reference to results—is less rare, though likewise not so common, as had been supposed. But the imitation of an act because that act accomplished a certain result, and in order to accomplish the same result—this was not found, though this is the kind of imitation assumed in current explanations to be common. The extensive experiments of Hobhouse,[2] evidently undertaken with the expectation of finding imitation playing a part, are striking as an example of how

[2] Mind in Evolution, Chapter VIII.

little it is possible to find, and how uncertain is what is found, even with the best of will. Kinnaman, Small and others had incidentally seen a few examples of real imitation. We have now from the Harvard laboratory two careful studies of this matter by Berry,[3] with results that are most interesting. In Berry's rats and cats we find imitation as it were in the making. Our conception of imitation, and of its different kinds, loses its sharp lines and angles and becomes indefinite. When one knows how to escape or get food and another does not, the animals do not set to work to imitate each other's actions in the clear-cut way we are apt to think of as imitation. But the one that doesn't "know how" does after some time begin to pay attention to his comrade's actions, and then in an indefinite way to do something of the same sort himself. "We found that when two rats were put into the box together, one rat being trained to get out of the box, and the other untrained, at first they were indifferent to each other's presence, but as the untrained rat observed that the other was able to get out, while he was not, a gradual change took place. The untrained rat began to watch the other's movements closely; he followed him all about the cage, standing up on his hind legs beside him at the string, and pulling it after he had pulled it, etc. We also saw that when he was put back the immediate vicinity of the loop was the point of greatest interest for him, and that he tried to get out by working at the spot where he had seen the trained rat try."[4] In cats similar and more marked cases of imitation were found and analyzed.

Berry's work is the first really scientific study of imitation in animals that we have had, and it shows, as so commonly happens when a thorough study is made, that we can not make extreme statements, whether positive or negative. Imitation is found; even "reflective imitation," but it is not precise; we can often hardly be certain whether it *is* imitation; and where it is more pronounced it is difficult to distinguish imitation for the mere sake of doing what a companion does, from imitation for the purpose of accomplishing the result that the companion accomplishes. Like all other traits of behavior, imitation grows gradually out of something that seems not the same thing at

[3] Berry, C. S. The Imitative Tendency of White Rats. *Journ. Comp. Neurol. and Psychol.*, 16, 333–361. *Id.*, An Experimental Study of Imitation in Cats. *Ibid.*, 18, 1908, pp. 1–25.

[4] Berry, The Imitative Tendency of White Rats, p. 358.

all! Whether to call this "something" by the same name as the developed activity is one of the frequent grounds for unprofitable controversy.

L. W. Cole[5] has made an elaborate investigation of the intelligence of raccoons, with results of more than usual interest. The raccoons are compared throughout with the famous cats of Thorndike, and the work, like most recent work on animal intelligence, follows the outlines of the well known paper of the author just mentioned.[6] But Cole has made real and important advances in both method and results. The raccoons are either much more clever than the cats, or the methods employed were better fitted for bringing out latent possibilities; probably both these things are true. The experiments consisted largely in allowing the animals to learn to open boxes closed by fastenings of various degrees of complication. The raccoons learned somewhat more readily than the cats. As in all other animals, their learning was largely by trial and error. But they are not restricted exclusively to that method, as Thorndike maintained to be the case for the cats; decidedly not if we limit that method to the *gradual* formation of an association between a motor impulse and a sense perception. (1) There was clear evidence that the animal at times, catches the idea, that a certain act is what opens the door, so that he later acts directly and at once on that idea. It is not a mere gradual exclusion of useless movements, till only the useful ones are left. (2) The raccoon learns by being put through an act. It learns without "innervating its muscles," the great test for the possibility of learning in Thorndike's cats. It learns to go into a box by a certain entrance, through having been *lifted* into the box that way a number of times. By being put through them, it learns certain acts which it was unable to learn by its own efforts. By putting different raccoons through the same act in different ways, they learned to perform it in different ways; for example, one learned to lift a latch with its paws, another with its nose. (3) While the raccoons, like most other animals, do not imitate each other or any one else in a marked degree, they did, after seeing the experimenter perform a certain action many times, "catch the idea" and endeavor to perform the action for themselves. This

[5] Cole, L. W. Concerning the Intelligence of Raccoons. *Journ. Comp. Neurol. and Psychol.*, 17, 1907, pp. 211–261.

[6] Thorndike, E. L. Animal Intelligence. *Psychol. Review*, Monograph Suppl., vol. 2, 1898.

is of course the essence of "reflective" imitation. (4) Thorndike concluded that cats have probably no "free ideas"; no stock of images which are motives for acts. The association in the cats was always between a motor impulse and a present sense perception; there was no association *of ideas*. This negative conclusion was based largely on the inability of the animals to learn from being put through an act. In this latter matter, Cole calls the reader's attention particularly to "*the radical difference at every point*" between the cats of Thornidike's experiments and the raccoons of his own. "If inability thus to learn is evidence against the presence of ideas, then ability to do so should be equally strong evidence for it." Furthermore, Cole gives much additional evidence for the presence of ideas in the raccoons; and certain results of some extremely ingenious experiments amount to a demonstration that the animals do hold mental images, so far as such a thing can be demonstrated.[7] The animals seemed to remember definite objects for a time, then forget them; then suddenly, under certain conditions, recall them. They fought against being put into boxes with complex fastenings, from which they had some time before had difficulty in escaping, though they willingly went into similar boxes whose fastenings they had found simple. In certain experiments there were two alternative signs to be raised; the green one meant food, the red one meant none. The raccoons learned to raise these signs by clawing at the standards, but they could not see beforehand which sign would come up by clawing at a certain standard. *When the red one came up they clawed it down again,* then clawed up the green one, and made ready to receive food. Clearly, the red sign did not correspond to an image that the animal had in mind, while the green one did. Other experiments were devised in which success depended on the animal's holding in mind the images of certain things that had gone before; the raccoons stood these tests successfully. It is difficult to see how there could be more conclusive proof of the presence of ideas in animals that can not talk.

[7] A subjective thing, such as an idea, can, of course, not be absolutely demonstrated by objective methods. It is always possible to substitute for the idea its physiological accompaniment, and say that this is all that we can be assured of. In other words, "demonstration of the existence of ideas" in animals can never go further than to show that *they act as men do when men have ideas.*

The interesting paper of Hamilton[8] is perhaps in its origin independent of the Harvard laboratory. We have seen that the dancing mouse learns to act on the basis of a comparison between two things, selecting, not a particular thing, but the lighter of two, or the darker of two, etc. Kinnaman found that the monkey could similarly learn to choose always the lighter vessel, or to choose the colored vessel from among a number of vessels, even when the colors were changed. Hamilton made a precise study of a similar sort of action in a dog. The animal learned that in order to escape from a pen and get food he must press, out of a number of levers, the one that bore the same sign that was found on a general sign-board elsewhere in the pen. In successful cases his method of procedure was, then, to inspect the general signboard, then to pass in review the four levers till he found the one that bore the same sign—then to press this. This appears to involve a fairly complex mental operation (if we may venture to interpret the actions of animals from that highly reprehensible standpoint). The dog clearly learned to choose in the way described. But unfortunately, being a clever dog, he after a time discovered a much simpler method of action that accomplished the same results. He merely began at one end of the series and pressed the levers in order till he came to the one that worked. When electric shocks were attached to the "wrong" levers, he decided that he didn't care to play at that game any longer, and the experiments had to end.

How far such action, seeming to involve complex mental operations, may be demonstrated in animals when there has been fifty years' development of method and results in such investigations, instead of merely two or three attempts at it, is a question that deserves consideration by those who are so ready to deny, on the basis of what we now know (or rather, on the basis of what we don't know), all mental complexity in animals.

Indeed, it is clear that much of the work we have just reviewed consists in showing experimentally that the mental operations of animals are more complex than had been supposed; in restoring to animals certain things that had been denied them. And this is typical. The recent history of the study of animal

[8] Hamilton, G. van T. An Experimental Study of an Unusual Type of Reaction in a Dog. *Journ. Comp. Neurol. and Psychol.*, 17, 1907, pp. 329–341.

behavior has shown a curious parallelism in each of its three great divisions. In each division the slate was, as it were, wiped clean some ten or fifteen years ago; the existing structure was razed to the ground, and we have been building it up again ever since. In the lower organisms Loeb reduced the phenomena to almost inorganic simplicity. For the ants, bees and other higher invertebrates Bethe took similar action; they were stripped of their fanciful decorations of memory, intelligence, etc., and left absolutely devoid of "psychic qualities" of any sort; their behavior was composed of invariable reflexes and tropisms of the simplest character. Thorndike performed the same operation for the vertebrates. Not only did they not reason (preposterous notion!), but they did not imitate, could not learn by seeing a thing done nor by being put through an act, nor by any other way than by simply gradually dropping out useless movements from among those made at random; and they had not even *ideas* of things past, to say nothing of perceiving relations or being capable of trains of thought or of formulating a plan.

In all three divisions of the subject the work since these operations has consisted largely in the slow and painful restoration, by precise experimental methods, of what was thus wiped out at one fell swoop. The three authors named, with those that aided them, perhaps did the science of behavior the greatest possible service at that time. Before them there was hardly an ordered science in this subject; there was a jungle of suppositions, assumptions and anecdotes. Loeb, Bethe, Thorndike and Company destroyed all this and compelled us to rebuild from the ground up, a solid structure, based on precise scientific methods. How high the structure will have to go, no one can foretell; certainly it is not yet finished. Indeed, animal behavior as a science is merely in its swaddling clothes; it can not carry as yet many sweeping conclusions, particularly negative ones. General negations based on what we now know are most unscientific; they are largely capitalizations of our large stock of ignorance. It behooves the man of science, therefore, to be careful in his destructive criticisms; some recent controversies show that this caution is much needed. It will be long before our science is coextensive with the phenomena with which it is attempting to deal.

H. S. JENNINGS.

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